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so small a portion of the whole, that a little inequality in the poise or a moderate wind has no sensible disturbing effect. The clock will gain or lose a little as the power alters the arc of vibration of the pendulum; but this will not be much, and may, perhaps, be sufficiently compensated by adopting a scapement between the recoil and dead beat.*

Observations of the Satellites of Uranus, made with the 20-feet Equatoreal. By Mr. Lassell.

"In the first column of positions, the angles are given with reference to the parallel of declination, from the north point round by east; and, in the adjacent column, the positions are approximately reduced to the angles on the apparent orbits, reckoned from the ascending node round by north in the direction of the motions of the satellites.

"The proportion of the transverse axis to the conjugate, as the orbits are at present projected, is nearly as 8 to 6, and the inclination to the ecliptic is about 82°½ s. p. and n. f.

"In every instance the positions and distances of (1) and (2) are the results of *estimations*,—generally from the *measured* positions and distances of the two bright satellites. And the first two satellites discovered by Sir Wm. Herschel in 1787, usually termed "the 2d and 4th," or "the bright satellites," are here designated by the Roman numerals III and IV.

	(1.)			(2.)			III.		IV.	
	Positions.			Positions.			Positions.		Positions.	
Green. M.T.	Paral^l.	Orbit.	Dist.	Paral^l.	Orbit.	Dist.	Paral^l.	Dist.	Paral^l.	Dist.
1851.	°	°	″	°	°	″	° ′	″	° ′	″
Oct. 24·4375	163	275	...	354	77	...	168 26	30·83	156 11	39·92
28·5	325	115	...	350	84	...	358 0	29·92	33 0	...
30·4791	35	30	10	169	268	...	268 9	23·28	350 23	40·99
Nov. 2·5	320	121	12	273	163	13·8	160 43	28·91	272 47	29·39
12·4375	332	106	...	131	311	...	112 34	...	5 0	...
17·4375	337	101	...	47	19	...	257 34	24·09	220 12	34·63
18·5	186	242	...	316	126	...	202 0	...	191 6	36·83
21·4889	123	318	...	63	7	...	91 7	21·38	131 57	34·29
22·4833	332	106	...	329	111	...	38 50	26·03	101 32	...
27·4798	345	91		not seen			194 25	18·61	325 31	38·12
Dec. 11·3750	150	291		not seen			340 0	...	304 0	...
16·4132	160	280	13·5	70	0	14	147 0	...	172 19	...
22·3541	...	...	...	...	...	...	248 26	...	10 2	39·46

Observations and Remarks.

Oct. 24. Sky pretty favourable; the new satellites (1) and (2) seen certainly for the first time.

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* In extreme cases, the pendulum might be suspended from a string between cycloidal cheeks, as in Huyghens' clocks.

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Oct. 28. The sky had only recently cleared at 11½ hours, after a rainy and stormy evening; and though the new satellites were certainly seen, the opportunities afforded between the passing clouds only sufficed for a careful diagram of situations, and a single measure in distance of Satellite III.

Oct. 30. An exceedingly faint star was noticed, distant about 3½′ and position about 150°, but no opportunity occurred of ascertaining whether it was or was not a satellite The measures of the old satellites good and satisfactory.

Nov. 2. Sky stormy and many clouds passing, with a disturbed, though transparent, atmosphere. Exactly at midnight (2) was in the line joining the centre of *Uranus* and IV, and at rather less than half the distance of IV from the planet. At a later period of the night it had evidently passed that line of conjunction.

Nov. 12. Sky hazy, and the faint satellites seen only with great and painful attention. No *measures*, except a single one of III in position, could be got.

Nov. 17. Very keen frosty wind; air indifferent, yet the measures, laboriously taken, are, I think, good. No other stars around *Uranus* at all likely to be satellites. I surveyed the planet some time, with a wheel of, I believe, very excellent concaves, made for me by Mr. Andrew Ross; principally using powers 540 and 351, as they seemed most efficient: but the faint satellites scarcely could be recognised with them, and certainly they were much less evident than with the positive Munich eyepiece 614, or the (Ross) Huygenian 778. I cannot, therefore, corroborate the impression that concaves are more luminous than other eyepieces, in a trial which I should almost think an *experimentum crucis*. I tried the various powers of the wheel up to 2300; and on other occasions I have made desultory comparisons with concaves, leading to a similar result. They perform best when the powers are low.

Nov. 22. At 9ʰ 30ᵐ, (2), generally the faintest, is nearly in conjunction with (1), or rather a little in advance of the right line joining (1), and the centre of the planet. Atmosphere variable and frequently hazy; but the measures obtained are worthy of confidence. On this occasion the full aperture of the telescope was more efficient than any reduced aperture in facilitating the measures of both positions and distances.

Nov. 27. Measures pretty good, though I was unable to obtain a certain view of (2), the faintest. On calculating afterwards I found that it would be then at about its nearest approach to the planet.

Dec. 11. The air so exceedingly unfavourable that I could only make a diagram of the situations of three of the satellites, (2) being again invisible.

Dec. 16. All the satellites well seen for a short time, but clouds coming on prevented more than a careful diagram being made, and a single measure of IV being obtained; III and IV equal in brightness, (1) very decidedly fainter, yet brighter manifestly than (2).

Dec. 22. Vision very indifferent, yet the measures, carefully taken, are, I believe, good.

"The most probable periods of revolution I have been able to deduce for the new satellites from this series of observations are, $2^d\ 12^h\ 18^m\ 58^s$ for (1), and $4^d\ 3^h\ 33^m\ 26^s$ for (2). With these data I have been led to look back to the observations I made of suspected interior satellites in 1847, and published in the *Monthly Notices*, vol. viii. p. 44, but, by a typographical error, headed 1848 instead of 1847. On reference to the original observations and diagrams, from which the angular positions were deduced, I find that a position-angle of 350° on the 14th September better suits the diagram than 10°. Also, that the observation of 29th September is very questionable, there being only a rude diagram made, without any confirmatory remark appended: I therefore

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reject it. But the night of the 6th November was remarkably fine, and a very careful diagram was made of the four satellites in respect to position and magnitude; and remarks are appended which sufficiently attest the correctness and certainty of the observations. I find, however, that the diagram has not been correctly translated into angular positions; and, rejecting altogether the observations of the 29th September, a table of the series will stand thus,—

	(1.)		(2.)		III.		IV.	
	Positions.		Positions.		Positions.		Positions.	
	Paral^{l}.	Orbit.	Paral^{l}.	Orbit.	Paral^{l}.	Orbit.	Paral^{l}.	Orbit.
1847.	°	°	°	°	°	°	°	°
Sept. 14·567	350	84						
27·408	326	114						
Oct. 1·521			348	87				
Nov. 6·429	346	89	126	315	292	148	344	92

"Presuming that on the 6th November, 1847, I obtained undoubted observations of the four satellites,* and that (1) was observed on the 14th and 27th September, and (2) on the 1st October, the respective periods given by comparisons of these observations agree with the recently deduced periods within very small limits of error. And comparing the observation of 6th November, 1847, with that of the 2d November, 1851, interval = 1457·071 days, and concluding that 580·114 revolutions of (1) took place in that interval, I obtain for its period 2·5116998 days, or 2^{d} 12^{h} 16^{m} 50^{s}·86. In like manner, concluding that 351·5694 revolutions of (2) were performed in the same time, its period comes out 4·144477 days, or 4^{d} 3^{h} 28^{m} 2^{s}·91.

"In October 1847 I estimated the proportions of the axes of the orbits to be as 10 to 5·15, and that the major axis was inclined to the ecliptic 87°½."

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* *Verbatim Copy of the remarks entered in my journal, under date 6th November, 1847, with a tracing of the diagram contained therein.*

Uranus 9^{h} 35^{m} M.T.

"The most distant about six diameters, the other about three. The closest satellite observed at 10^{h} 20^{m}. There is no question whatever of its visibility. Though fainter than the next, perhaps from its closeness to the planet, distant about two diameters, barely. A still closer satellite following, detected at 10^{h} 50^{m}: by much the faintest, but yet undoubted. At 11^{h} 30^{m} the satellites retain nearly the same position with respect to *Uranus*, which proves them to be satellites, as the planet would otherwise have left them behind. I have been repeatedly interrupted by thin clouds, and they are now again forming, so that I fear I shall not get a measure. I think the faintest satellite is distant 1½ diameter, and that the next is something more than two. 11^{h} 40^{m}, it has become completely cloudy.